A

REFUTATION

OF THE

REMARKS

ON THE

INSTITUTES OF

Experimental Chemiſtry:

By Robert Dossie

IN A LETTER

Addreſſed to the Members of the Society for the Encouragement of Arts, Manufactures, and Commerce.

"*Ut jam nunc dicat; jam nunc debentia dici*
"*pleraque differat, & præſens in tempus omittat.*"

HORAT. Art. Poët.

LONDON,

Printed for J. NOURSE in the Strand. 1760.

TO THE

MEMBERS of the SOCIETY

FOR THE

Encouragement of Arts, Manufactures, and Commerce.

Gentlemen,

I Beg leave to submit to your consideration this defence of myself and my writings, from an attempt to disparage and depreciate the one through the other: and, by that means, to lessen my pretensions, as a candidate, to the office of principal secretary to your society. I do not, therefore, lay the subject of this letter before you meerly in a literary light; which possibly might not appear so pertinent to you: but as the occasion of the dispute in question arises solely from my being a candidate to an office in your election, and the ground of it is an attempt to influence your opinion of me in relation to that concern, I hope the addressing myself to you in this view, will not be thought improper. To you, as the most fit tribunal, I consequently appeal, with the greatest confidence in your justice and candour. The same spirit of benevolence and patriotism, that links you together as a society, solely in the design of advancing the interest of your country, will, I doubt not, operate also, relatively to a private case, in the breasts of each individual;

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and inſpire a due ſenſe of equity to decide againſt, and concern to redreſs, the injury I complain of. Every honeſt man muſt be offended at the appearance of *bad* deſigns: and every gentleman muſt be diſguſted with thoſe means, for the proſecution of *any* deſign, that are *inſidious* and *deceptive*.

The particular ſubject, which occaſions this addreſs to you, is an attack made on my character, with reſpect to abilities, in a pamphlet intitled, *Remarks on Mr. Robert Doſſie's Inſtitutes of experimental chemiſtry:* a work ſome time ago publiſhed by me: which, together with two other treatiſes of my compoſition, are there cenſured and condemned, in the moſt outrageous manner, under the ſanction of pretended reaſons, that are, for the moſt part, founded on falſe repreſentations, and miſquotations of what I have advanced. In order, however, to ſet the ſtate of the caſe in a clear light, it is previouſly requiſite, to ſhew under what circumſtances, and in what manner, with relation both to theſe works and myſelf, this injurious piece of criticiſm was produced: that the motives of its publication being more evident, it may the more obviouſly appear, what weight and authority the facts and doctrines compoſing it ought to have with thoſe, who are not qualified, by a prior attention to this kind of ſubjects, to judge with certainty of particulars.

The Elaboratory laid open, and the Handmaid to the Arts, were both publiſhed in the year 1758, and the Inſtitutes of Experimental Chemiſtry

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about nine months ago. They were all given to the world without having any name prefixed to them; or bearing any other mark by which I might be known to be their author. The reaſon of this concealment, did not however ariſe from any conſciouſneſs, that they contained what I ought to be aſhamed of; but from my conceiving that they might equally well anſwer the end of their publication, which was, the propagating the knowledge of the ſubjects they treated of, without being the acknowledged work of any particular perſon: and that I might by this means avoid any hazard of being under a defence of my own character, in relation to them, from the cenſure and objections, that either prejudice againſt the opinions advanced in them, or perſonal diſlike to myſelf, might give riſe to. If they had merit to claim the approbation of the world, I preſumed that merit would ſooner or later procure it: but as it related to myſelf, I had little ſolicitude about the event; as I had not then any particular views, in which it could be of much conſequence to me. The public was, however, pleaſed to be favourable to them far beyond my expectations. All the Engliſh writers of Reviews, and other ſuch periodical works of criticiſm, had the candour and generoſity to recommend them in the ſtrongeſt manner. The authors of a foreign work of a parallel kind, were alſo equally indulgent to one of them: and I had many concurrent teſtimonies of a private nature, of their being as well received as I could wiſh, by thoſe, to the aſſiſtance of whoſe ſtudies, or practice, they were particularly intended. I flatter myſelf, from all theſe circumſtances, they would ſtill have enjoyed the ſame advantages, had their author been yet unknown; and that they would have remained uncenſured, had they continued anonymous. But an occurence, of which you are well apprized, induced me lately to declare myſelf the author of them: and afforded the ſame motives to others to cenſure, or rather abuſe them, as to myſelf to claim them. Your ſociety thought proper to inſtitute the office of a principal ſecretary; and many of my friends intimated their opinions to me, that they thought me a fit perſon to fill it; and urged me to offer myſelf a candidate: to which having conſented, it was thought neceſſary, that I ſhould avow ſuch of the works I had publiſhed, as had any relation to thoſe ſubjects, which make the object of your ſociety's conſideration; as you might from thence have the moſt certain method of judging of my qualifications. This was more particularly expedient; becauſe one of them, the HANDMAID TO THE ARTS, treated expreſsly and ſolely of thoſe matters, which make the object of your care and encouragement; and was publiſhed with a deſign ſo correſpondent to yours, that I dedicated it to you; and pointed out, in the preface, ſome very ſtrong political reaſons for the inſtitution of ſuch a ſociety; which had not, to my knowledge, been before offered to the public. In making this uſe of the works I had publiſhed, I employed none of thoſe artifices,

fices, modernly called *puffs*, in order to prepoſſeſs; or even draw your attention to them. My name was only put to the advertiſement of the books themſelves; and not the leaſt advantage was offered to be taken of them in the others, where I declared myſelf a candidate: there was indeed but one repetition of that of the books after the others were inſerted; which was by accident only, and in a different part of the paper. I meerly ſignified there, that I offered myſelf a candidate for that office, without intimating any ſuperiority of pretenſions I had over my competitors, either from qualifications of this kind, or from my not being engaged in any other employ: as I thought, it might ſeem to intimate invidious compariſons: though from the ungenteel treatment which I have ſince met with, I find my delicacy in this point might well have been ſpared. But no ſooner was my name put to theſe works, than the moſt ſtrenuous attempts were made to diſparage them, in order to prevent the effect they might have, in pointing me out as a proper perſon to be the ſecretary then in queſtion. In order to this, a junto of perſons, induced by various motives of paſſion or intereſt, whoſe names I ſhall ſpare the expoſing here, ſet themſelves down in judgment on the INSTITUTES. But not being however well acquainted with the ſubject, nor, as will appear from their performance, when carefully examined, very acute in their general conception of philoſophic matters, they failed to find an adequate number of real errors and inaccuracies to anſwer their purpoſe of ſhewing the work defective in the degree ſought for. In default, therefore, of ſufficient ground of cenſure from juſt remarks, they had recourſe to artifice; and by blundering over, and miſtaking the ſenſe of ſome paſſages, and deſignedly miſrepreſenting others, together with poſitive contradictions of demonſtrable truths, and unfair cenſures of the general deſign and manner of execution of the book unſupported by any aſſigned reaſons, they formed a ſyſtem of abuſe and impertinence, calculated to impoſe on, and prepoſſeſs, ſuch as had either not read the work: or, from their being little converſant in ſubjects of this kind, were not adequate judges of the points in queſtion. This they reduced to the form of a pamphlet, in the manner of a *letter addreſſed to the authors of the Review*, &c. under the pretence of reproving them for their falſe accounts of my works: avoiding, nevertheleſs, to uſe the *plural* of the word *review*, leſt it ſhould appear, that more than one had, by their approbation, been guilty of the crime that called for this ſevere reprehenſion; though in fact all the writers of the periodical works of criticiſm, then ſubſiſting, had been equally culpable in this point. But before this work was completed, part of it was ſhewn in manuſcript as diſcoveries of my ignorance, or want of veracity, made by a gentleman who had really, by experimental examination, found my aſſertions to be falſe: and the warm friends of the other candidates, as well as thoſe of him on whoſe account it was written, were not idle in taking

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advantage of it, by inſtilling prejudices againſt me on this ſcore. The pamphlet was, nevertheleſs, in due time printed : but, as an open publication was not the beſt method of rendering it effectual to its intended end, it was for ſome time only ſhewn privately to thoſe whom it was deſigned to miſlead. For had it been publiſhed before the end of the inſtant month, the review writers, in their own juſtification, might have ſhewn it in its true colours ; and prevented, by that means, the effect hoped for from it. In due time, however, when that danger was over, it was publiſhed, if the once advertiſing it in the public papers can be called ſo : but as this was for appearance only, and the members of your ſociety, or their friends, and not the world in general, were intended to be the readers of this work, it was judged a more effectual method to diſperſe the copies, by giving them away gratis, than to truſt to their ſale in pamphlet-ſhops. Accordingly they were ſent to the houſes of ſome gentlemen, and given to others at the ſociety's meetings ; and every opportunity was taken by the junto, and thoſe who could be induced to concur in their meaſures, to ſpeak of this rhapſody of nonſenſe, as containing a full demonſtration, that the *Inſtitutes of Chemiſtry* was a weak, abſurd, and contemptible performance ; and, to uſe the words of the preſident of the junto, " that the author was no chemiſt." As this manner of proceeding is too groſs to have any effect, after it is in the leaſt underſtood, and the contents of the pamphlet abſurd to impoſe on thoſe whoſe taſte for the ſubject, to which it relates, would incite them to take the pains of comparing it with the book, I ſhould at any other time have treated ſuch an attack with contempt and neglect: But as my being a candidate for an office in your ſociety draws the attention of many on me, who not being converſant in theſe matters, would be otherwiſe inattentive to them, the momentary impreſſion which may be made on ſome of the members, by the confident aſſertions, and impoſing tone of the writers, becomes of conſiderable conſequence; and lays me under a neceſſity of juſtifying my work, and preventing even the temporary effects. Since before juſt notions of this affair can be propagated in private converſation, by thoſe, who being really adequate judges, may ſet the matter in a true light, the injurious conſequences intended, would have taken place in the influence, the perverted notions of ſome members of your ſociety might have on the election. It was therefore incumbent on me to give ſome anſwer to theſe fallacious remarks: and to point out, by ſuch means as might have tendency to convince even thoſe not verſed in theſe matters, what the true ſpirit and intention were, that animated the writers to undertake this work; and what the means were by which they have proceeded in the execution of it. In order to this, I ſhall therefore principally confine my obſervations to ſuch points as do not require the knowledge of abſtract principles, nor an acquaintance with nice and complex experiments, to the forming a decisive judgment: and these consequently are such as regard rather the conduct and manner of their work, and their unfair treatment of mine with respect to false quotations, and perversion of the sense, than the discussion of particular doctrines, or examination of facts. Though I shall not omit to touch on some of the more glaring instances of their absurdity and want of veracity, even with respect to those. In the performance of this, to give a more conclusive view of their failure in what they pretend in either way, I shall not follow them through each article, according to the order in which it stands in the pamphlet; but first consider the manner of their work, and what they have said against the general plan and conduct of mine; and then descend to some few of the most notorious particulars.

The singularity of the title, *Remarks on Mr. Robert Dossie's Institutes of experimental chemistry*, is the first thing which strikes the reader with some degree of surprize; and presents, when duly attended to, a sufficient proof of the intention of the book. The unusual manner of putting the author's name, as well the christian as surname, before that of his work, may seem at first only a contemptuous insult; but, on reflection, it will soon be found to have another design also. By rendering the name so conspicuous, the attention of the members of the society, who pass by the windows of shops, or other places where it is put in view, is drawn to the pam-

pamphlet: as it indicates to them, that the contents of the book, though reſpecting ſubjects for which they would otherwiſe have an abſolute indifference, relates to a candidate for their ſecretaryſhip: which inſpires neceſſarily a curioſity for further information regarding it; and they are thus induced to ſwallow the bait. So that this peculiarity in the title, inſtead of an impertinence, is, in reality, an artifice truly worthy of the junto who deviſed it. The form of the work preſents likewiſe a like inſtance of the low, but ſhallow, cunning with which it is compoſed. To write without any ſeeming reaſon, but meerly to diſparage a perſon then a candidate for a public office, might have too barefacedly evinced the intentions: A pretence was therefore borrowed from the ſuppoſed miſbehaviour of the writers of the Reviews, with reſpect to chemical authors: though part of the criminal tranſactions, for which they were now ſo ſeverely reprehended, had gone unpuniſhed for near three years; and the other part many months; during all which time the public had been ſuffered to be impoſed on, notwithſtanding that great zeal of theſe writers for truth, which now burſts forth ſo ſuddenly and violently. But, according to their own relation, they forbore to interpoſe ſooner; becauſe they preſumed, from the falſe account the review writers had before given of the *Elaboratory laid open*, and the *Handmaid to the Arts*, they would give a very juſt one of the *Inſtitutes*. " *I ſhould have ſent you ſome*

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animadverſions on them as they made their appearance, if I had not thought yourſelves more equal to the taſk:" ſee page 17. How clumſey, how inexpert are theſe writers in their own paltry arts of deception, not to be able to give even a moderate ſhare of plauſibility to their pretences, but to blunder, and fall into inconſiſtencies, as well in their introductory plan, as in almoſt all the particular ſubjects of fallacy it was intended to exhibit! Theſe circumſtances would be too minute and trivial to touch upon on any other occaſion: but at preſent they are more material, as they diſplay and illuſtrate the true ſpirit of the work; and evince the real intention of the writers with equal force, as paſſages of ſeemingly more importance.

But to proceed to the particular contents. The firſt thing that is exhibited by them to our obſervation, is ſuch an eagerneſs to condemn, joined, as appears, to a total ignorance in philoſophic ſubjects, as leads our cenſurers into a moſt complicated abſurdity in their very entrance into criticiſm. They ſet out in ſuppoſing themſelves, or at leaſt intimating to others, that I have offered this work as a "*body of chemical philoſophy,*" though I have called it *Inſtitutes of experimental chemiſtry*; and expreſly declared, that I have not extended philoſophic ſpeculations beyond what related to particulars, further in any part, than was neceſſary for the underſtanding the reaſon of the proceſſes in the experiments. But to this blunder they were led, I ſuppoſe, by my mentioning in the pre-

preface the importance, and, indeed, neceſſity of ſuch a work, in order to the further improvement and progreſs of natural knowledge: and intimating my entertaining ſome thoughts of ſuch an attempt hereafter. When they have however preſumed this work, contrary to my own account of it, to be offered as ſuch "*a body of chemical philoſophy, founded like the mechanical on general principles,*" they ſay, "*what he calls general principles, are deduced from particular facts, and by being made general, they are made falſe.*" Can there be a more evident proof than this extraordinary charge of the total want of abilities of theſe writers to intermeddle in philoſophic matters: or of their unparalleled preſumption on the ignorance of their readers? For from whence are general principles in natural philoſophy to be collected but from particular facts? Is not this the method, by induction, introduced by my Lord Bacon in the place of the ſophiſtry and verbal ſyſtems of the ſchools, and adopted by Sir Iſaac Newton, who lays it down as the ſole baſis of philoſophic knowledge; and by all others who have ſucceeded in the inveſtigation of phyſical truths? Whether therefore are we to wonder at, the hardy ignorance of a writer, who cenſures another at this period, for perſuing a method ſo obviouſly right, and univerſally received: or is this only an artifice to impoſe on ſuch of the readers, as are not converſant in matters of this kind, in order to anſwer the particular purpoſe which this pamphlet was intended to ſerve?

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Their poſition, that *principles deduced from particular facts* are made *falſe* by being made *general*, is equally extraordinary. It ſeems indeed a contradiction in terms. For the principles themſelves are no more than the relations of analogy, which the particular ſubjects have with reſpect to facts collectively conſidered: and, therefore, as far as they are general, they are neceſſarily true. But I ſuppoſe theſe writers, had they been able to expreſs themſelves intelligibly, would have ſaid *univerſal* inſtead of *general*, as may be gathered from the inſtances by which they endeavour to prove what they aim at charging me with; and then indeed had I ſet up ſuch principles, the charge might have had ſome foundation: as the analogical link of relation in the properties, as well as forms of particular kinds of bodies, are ſometimes interrupted. But I have been ſo far from ſetting up *a chemical philoſophy founded like the mechanical* on principles that hold good *invariably* and *univerſally*, that an expreſs paſſage in my preface, and ſeveral others in the body of the work, evince, how cautious I was, not to give room for miſtakes of this kind. In the preface I ſay, in ſpeaking of the nature of that general knowledge we may have of the properties of bodies with reſpect to their action on each other ſpecifically conſidered "That the principles of this knowledge *do not extend to* all *inſtances with equal certainty, as in the caſe of the mechanical: but yet* hold good in a degree, that makes ſuch *an approximation, as anſwers*

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extremely well in practice, and makes, moreover, a body of ſpeculative ſcience, &c." Preface, page vii: and in page 14 of the work itſelf alſo, ſpeaking of the ſpecific attractions of bodies, it is ſaid, "*Moſt ſpecies of bodies, which have ſo much the ſame qualities in appearance as to be deemed of the ſame genus in the general view of nature, have, for the greateſt part, the ſame ſpecific attractions: though this is not without ſome abſolute* exceptions *to the contrary; and great* variations *in the degree of almoſt all from each other.*" Indeed, in every part of the work, it is inculcated, that the generical relations of bodies, which make the principles of the chemical philoſophy, are never to be conſidered as univerſal: but only as *general, with limitations*; and that the proper object of this ſcience is, the demonſtrating the mode of relation which is general: and then noting the ſeveral deviations and exceptions from it. Again, in page 4, Vol. I. where I ſpeak of the method of applying the experiments of chemiſtry to the improvement of natural philoſophy, by reducing the ſubjects to genera and ſpecies, I expreſs myſelf, as will be found, in the moſt careful terms of limitation.

They endeavour, however, to produce inſtances of the failure of my attempt to eſtabliſh principles by experiments: which inſtances conſiſt partly in falſe facts, and partly in the miſtake or wilful perverſion of what I have advanced. Of theſe falſe facts, I ſhall take notice in their proper place: but with reſpect

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to misrepresentation of the sense; a passage that occurs here merits some notice. I have given a table to shew the subordination of the powers of bodies, of opposite genera, with respect to each other. This table is formed only in that view; to shew the relation the respective species of those two capital genera of bodies, called acids and alkalies, have, with regard to their subordination of attraction, or their power of departure, or dispossession of each other, from any third body of the opposite genus; and not to distinguish which, through the whole species of each genus, will act on any particulars of those of the opposite. But, in order to render the table defective, or faulty, they have confounded the disposition, or want of disposition to attract each other at all, with the comparative degree of force or power of such attraction, which any two may have with respect to a third: and as instances of my mistaking the degrees of attraction, have offered those species, which have attractions with respect to certain others, against those that have none, with respect to the same: as in the case of lead and mercury with the vitriolic acid, and several more. Whereas, there can be no comparison of the degrees of subordination of attraction, or power of dispossession, with regard to a third body, betwixt two others; of which the one will not at all combine with, or attract such third. The facts themselves likewise brought here, respecting these attractions, are most of them false, even considered in any light: as we shall have occasion to see in some inſtances below, when we ſpeak of their fallacy in relation to particulars: and were taken on the authority of Geoffroy's table, which is extremely inaccurate, and not on the experimental knowledge of the operative gentleman of the junto, or of another of ſuperior abilities, whoſe name has been likewiſe brought publicly in queſtion by them, as having a ſhare in this pamphlet. Though from the multiplicity of errors in point of facts, delivered in a work lately publiſhed by him, as from the diary of his own experiments, as alſo with reſpect to doctrines, he ought to treat the characters of other writers with tenderneſs. But theſe matters are of too nice a diſquiſition to anſwer the end of this reply: and I will therefore wave them, and paſs on to what is more ſimple, and generally intelligible; only I would beſpeak a little more candour and allowance on any future occaſion, for inaccuracies, in large works, that may depend on inadvertencies of expreſſion, from a junto of writers, who, notwithſtanding the magiſterial tone and judicial character they aſſume, can, in the ſecond page of ſo ſhort a piece, according to the literal expreſſion, call a *table* a *doctrine.* "*In the table of attraction (one of the fundamental doctrines of his ſyſtem) we are taught*, &c." *page* 2.

After this attempt to ſhew, by particular inſtances, that I have failed in forming ſuch univerſal principles as never entered into my imagination, theſe writers venture to advance, by a clear implication, a doctrine of their own: which is, that there are no general principles with

with respect to the specific action of bodies on each other. They say, "*even the deflagration of nitre with inflammable substances, than which nothing seems to bid fairer for a general principle, is, in our author's sense, by no means such: for if the nitre be melted, and a certain proportion of inflammable matters, as antimony freed from a part of its sulphur, be immersed in it, no deflagration will ensue.*" But what can be more weak and absurd, than either the position itself, or the instance by which they endeavour to illustrate it? Is it a consequence, that because the complex relation of a number of things to each other, is not universal, that therefore it is not general; or that because antimony, containing phlogistic matter, may not deflagrate with nitre, supposing it true, that therefore it is not a principle, that phlogistic bodies would deflagrate with nitre, if found, as he allows, to be so, in almost every other instance? Let me ask the *philosophic gentleman* who presides in this junto, if the polar attraction in MAGNETIC bodies ought to be exploded as a principle; because nice observers may discover some local variations in the effects: or, whether the principles of medicine, or other scientific arts, that depend on the powers of nature in minuter systems, stand on a better basis than those I have advanced. But to leave a matter too obvious to require further argument in the general view of it, and turn our eyes to the particular fact my adversaries have unhappily advanced here against the universality of the deflagration of nitre and phlogiſtic bodies; and the uſe they have made of it againſt me; I muſt take the liberty of ſaying, that in a philoſophic ſenſe, or, as they ſay, "*according to mine*," it is abſolutely falſe; as well as the application of it to their purpoſe. I never advanced that the *deflagration*, but the *commenſtruation* of phlogiſtic bodies with nitre, under certain circumſtances, was a general principle. For deflagration is no more than a term expreſſing ſome ſenſible effects of the commenſtruation of nitre and phlogiſton with each other, under thoſe circumſtances. And nitre, in many caſes, where it is demonſtrable from the effect, commenſtruates with the phlogiſton in compound bodies, without thoſe ſenſible appearances that are underſtood by deflagration. Thus particularly, when the quantity of phlogiſton in any compound body is ſo ſmall, that the action is not ſufficiently rapid and ſtrong to ſhew itſelf in the operation, theſe ſenſible marks of action will be wanting, though it be apparent in its effects: as in the very inſtance produced by our writers; where there is a conſiderable exploſive effect, while the antimony contains its full proportion of ſulphur, or phlogiſton; but, as this effect depends on the phlogiſton, it muſt neceſſarily be weaker as the proportion of that ſubſtance to the other conſtituents becomes leſs, till it be no longer ſenſible. But even then, when there does not remain a ſufficient quantity to make any detonation, or exploſive appearance; yet the ſame action evidently reſults betwixt the antimony, even in

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this ſtate, and the nitre, on their admixture in a due degree of heat: as is evident from its appearing afterwards perfectly calcined: which ſhews the action of the nitre on the phlogiſton, by the intire ſeparation of it from the metallic earth, with which it was combined in the conſtituence of the antimony.

The next article conſiſts of an attempt to render my work ridiculous, by applying another paſſage in the preface to the contents of it; when it is evident, by the ſubſequent lines, I had no ſuch meaning. This paſſage is thus. "*The author preſumes, that he has gone much* "*farther in the inveſtigation of the general prin-* "*ciples, on which nature conducts her operations in* "*the minuter parts of the ſyſtem, than any writings* "*already publiſhed lead;—and that he has made* "*ſeveral diſcoveries relating to particular ſubjects.*" This they ſay ſeems to have an eye to matters in different parts of the work, which they enumerate. But there can be nothing more wanting to ſhew the unfairneſs of their proceeding in the treatment of this book, by mutilation of the text, and perverſion of the ſenſe, than the quoting the whole of this paſſage: which is as follows.

"Preſuming, however, that I have gone "much farther in the inveſtigation of the general "principles, on which nature conducts her ope- "rations in the minuter parts of the ſyſtem, than "any writings already publiſhed lead; and that I "have made ſeveral material diſcoveries relating "to particular ſubjects, which could not be de-

"monstratively communicated, till a more just "and regular body of general doctrine than has "been hitherto laid down, were formed; I "thought it expedient, in order to prevent the "fruits of my labour, should they prove of any "value, from being wholly lost to the world, to "give, in the mean time, a more compendious "work; which may be preparatory to, and, in "some measure, substitutive for, one more co-"pious and perfect. For should the design of a "more complete system be at last abortive, this "might answer in a lesser degree the same end; "at least in rendering it practicable to me, to "treat hereafter intelligibly and precisely of those "particular subjects, in relation to which, I flatter "myself, I am enabled to give new lights that "may be of beneficial consequence."

Can it be more explicitly and clearly said, that the discoveries, here alluded to, were not a part of this book; but intended to be treated of hereafter? To have an opportunity, however, of raising prejudices against several parts of the book collectively, by a delusive form of censuring them as pretended discoveries, this false meaning is given to my words. But had I nevertheless intimated in this passage, that the book did contain several discoveries, even in very important points, as well philosophic as practical, considering every thing to be so that has not already been published in any other work than my own, I might perhaps be well justified in it: but modesty forbids I should enlarge on this point. The doctrines, which they have made a part of the detail of thoſe diſcoveries they treat with contempt, are, however, as far as they are aſſerted by me, ſupported by valid arguments in the parts of the work where they are delivered; to which I beg that recourſe may be had, by thoſe who are deſirous of being ſatisfied in theſe points, as they take up too much room to be recited here. As I would not, however, be wanting in candour even to my adverſaries, I beg leave to mention what appears to me ſome excuſe for their ſpeaking of them in this light manner. I mean their wanting capacity to underſtand them; which is evident, by their continual miſconception of things, even of a much leſs complicate nature. I ſhall refer to the work itſelf therefore, for the defence of my opinion, concerning the putrid and vital ferments in the blood; which, however it may be a *reverie*, according to theſe writers' witty turn, is not offered as a *theory*, as they fallaciouſly intimate; but as an *hypotheſis*, which it is expreſly called, that well merits conſideration. I ſhall do the ſame with reſpect to all thoſe other points of a general nature; where having alledged no reaſon for their ridicule or cenſure of them; nor diſplayed the particular manner of their own miſtakes about them, no anſwer can be given: unleſs that I hope, what I have ſaid will be found to be well warranted by the arguments, contained in, or annexed to, the very paſſages themſelves; or to be found elſewhere in the book. But as they enlarge a little, in relation to ſome ſuppoſed notions concerning animal and vegetable ſubſtances, I will endeavour, as far as the confuſion of their manner, and obſcurity and want of preciſion in their expreſſion, will ſuffer me, to follow them through that part. In ſpeaking of what I have meant in the paſſage of the preface above quoted, they enumerate among the other things, " that *I have an eye, to the project for explaining the phænomenon of the animal and vegetable œconomy, from the water, ſalt, phlogiſton, and earth, into which the parts of animals and vegetables are reſolved by putrefaction, and by fire.*" If by this they mean, a project to explain *all* the phænomena of the animal and vegetable œconomy on theſe, or indeed any other, principles, ſuch certainly never entered the imagination of any but themſelves. On the contrary, I have obſerved, in more than one part of the work, that, in the vital œconomy of animals and vegetables, nature acts by peculiar principles, that ſeem to break through the general analogical relations, which bodies have according to their reſpective generical properties. But if they mean, that it is ridiculous, to attempt to explain *any* of the phænomena of the animal and vegetable œconomy, from the known properties of water, ſalt, phlogiſton, and earth, the contrary is obvious: as may be ſeen by many paſſages of that work. They then proceed, however, to go deeper into this matter; and, in ſo doing, ſhew how much they are bewildered whenever they attempt to reaſon: and prove indeed, what they have below

low ſaid, " *that nothing can be more dangerous in chemiſtry than the ſpirit of generalizing*," to be extremely true, when taken with this limitation, " where nature has denied the power." The manner in which they thus enlarge on this ſubject, is as follows. " *The author himſelf admits, that by this ultimate reſolution or deſtruction, nearly all the parts are reduced into the ſame principles. How then can we deduce from thoſe principles, even their groſs differences, from one another, much leſs their different actions in the body of the living animal?*"

What with the defect of the grammatical expreſſion, and what with the inexplicitneſs of the ſenſe, it is impracticable to ſettle what they really intend here: whether to deny that all the parts of animals and vegetables are not reducible into the ſubſtances enumerated; or that the differences are not owing to the properties of theſe elementary bodies in the compounds: for either conſtruction ſeems equally abſurd: or do they mean, *by deducing*, explaining the modus of action in every caſe?

To ſay that theſe bodies, adding to them air, or ſome elaſtic fluid reſembling it, are not all the ſubſtances into which the parts of ſuch bodies are reducible, is a contradiction to what is known to all, who are converſant in experiments of this kind: and if theſe are all the ſubſtances, that enter into the conſtituence of animal and vegetable bodies, from whence can the differences found, be deduced, but from the combination of theſe bodies; their action on each other; and the new properties generated

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in the reſpective compounds? But the great difficulty here ſeems to lie in their ignorance of a moſt material point, from which they might have been free, had they read the INSTITUTES with a better deſign, than raiſing idle cavils againſt what they did not comprehend; which is, that many properties are generated in compound bodies, by the commenſtrual combination of the ſimple bodies or elements, of which they are conſtituted, that had no exiſtence in ſuch elements while in their ſimple ſtate: while others, which are found in the elements, are wholly ſuppreſſed in the compounds, as is explained, and demonſtratively proved, page 7, and 8, of the *Inſtitutes*. So that, from a few ſimple and elementary bodies combined in various manners, and proportions, an almoſt infinite number of compound bodies, may be produced; diſplaying all thoſe differences that make the object of this objection. But theſe writers illuſtrate this extraordinary notion, that the differences of animal and vegetable bodies, are not to be deduced from the properties of the bodies of which they are formed, by this argument. "*The ſame heat that liquefies the glutinous matter of the animal ſolids, coagulates that of the fluids: and what water, ſalt, earth, oil, and phlogiſton, can be diſcovered in the one more than in the other?*" If I underſtood the application of this query, in any manner conſiſtent with common ſenſe, I would give ſome anſwer to it: but it ſeems to me to imply, that the parts of the ſolids and fluids, being wholly compoſed of the ſame ſubſtances, have differences, nevertheleſs; and

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that, therefore, theſe differences muſt be owing to the properties of ſomething of which they are not compoſed. But this is equally rational with the paſſage that follows it: which is, "*the author indeed acknowledges that nature, in many inſtances, deviates from the laws he has eſtabliſhed: or, in other words, that he has himſelf deviated from the laws eſtabliſhed by nature:*" an interpretation truly worthy the candour, and good ſenſe of theſe writers: and which exhibits, in its true colours, the ſpirit of the whole. But this is ſupported in the uſual manner by a query; the force of which lies, according to the practice of theſe writers, in a ſophiſm, formed by the fallacious change of the word *deviation* into *repugnance*. "*How can he pretend to have diſcovered the principles on which nature conducts her operations, when many of theſe operations are directly repugnant to the principles he has advanced?*" By the ſubſtituting ſuch blunders and miſconceptions of the genuine ſenſe, as are found in ſome parts, and ſuch ſophiſtical commutations of words as are ſhewn in this and others, the moſt perfect work might be made to appear inconſiſtent and abſurd.

They afterwards obſerve, that, according to a miſquoted and mutilated part of my preface, I pretend to have ſettled the diſtinctions of the genera, and ſpecies of ſeveral of the kinds of natural ſubſtances, which had been either neglected, or unſucceſsfully attempted by preceding writers: and by a malicious inſinuation, ſupported by ſeveral pretended inſtances of failure in my attempts, they endeavour to take

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away all the merit to which this part of the work can have any claim. As I do ſtill inſiſt upon what I have advanced on this head in my preface; and believe I have reaſon to be ſatisfied, in the manner I there declare, with the reſult of my labours on ſo important a point, I will not paſs over what they have ſaid on this ſcore, without ſetting in a juſt light, the abſurdity and falſity of it. Their firſt objection lies againſt my ſpecification of earths, which I ſay is "to be "further incapable of analyzation or decompoſi-"tion; inſoluble in water; infuſible without vitri-"fication; incombuſtible; fixt in every degree of "culinary heat; and of a pulverine texture, or at "moſt to have only ſuch a ſlight degree of cohe-"ſive tenacity, as renders them very friable." Theſe qualities, taken collectively, give the true ſpecification of earths; and if any body in its *natural ſtate* want any of them, though it poſſeſs all the others, the abſence of ſuch quality affords an equally ſufficient mark of diſcrimination, or difference from, thoſe of any other genus. In order, however, to ſhew this ſpecification of earths to be faulty; and that it was applicable to bodies not to be deemed of the ſame, with any propriety, but belonging to another genus, they firſt made a falſe quotation, by ſuppreſſing the preceding words, particularly *very*, and ſaying only *friable*. And then affirm, that, according to this ſpecification, POWDERED platina is an earth. But could any perſon, except themſelves, have offered ſo ridiculous and weak a proof of the imperfection of my definition. The deſign, in laying down

down the generical characteriſtics, was to ſhew, what the apparent and interior qualities were, which the ſeveral ſpecies of each reſpective genus *have in common*, and which conſtitute them of ſuch genus. It is, therefore, ſuch qualities only as are *natural*, and inherent in the *proper and general form* of the bodies that can come in queſtion. But, in order to ſhew, that a body allowedly belonging to another genus, will fall under this deſcription, and thence prove that this definition fails in excluding all others, theſe ingenious writers practice an artificial operation. They *powder* the platina: and, when it is made to vary greatly from its *natural* form with relation to texture, becauſe it happens to coincide, in reſpect to other properties, with earths, it is brought as a proof of its being an abſurdity to ſay, that bodies, which in their natural ſtate are not tenacious and coheſive, but pulverine and very friable, are ſpecifically different from thoſe which are of contrary texture. But even with the medium of this confuſion of all principles, the matter would not turn out right without the uſual aid of miſquotation: for, if the words *very*, and thoſe preceding, had not been taken away from friable, the difference betwixt platina and earths would ſtill have been ſtriking; but in this, and moſt other inſtances, we ſee artifice joined to blunder. The choice of platina does not, however, ſeem very lucky for this purpoſe. For it happens to recede greatly, in its texture, from earths: and to be ſo far from being very friable, that

that it is, according to thoſe who have deſcribed it, not only very hard, but even, in ſome degree, malleable, when pure. I imagine, therefore, that ſome of the apyrous ſtones, which according to my doctrine, vary generically from earths, would have anſwered the purpoſe better with the help of *powdering*; but that operation would be ſtill neceſſary. One may reaſonably conclude from this paſſage, that theſe writers have cultivated ſophiſtry much more than ſcience: otherwiſe they could ſcarcely have fallen into ſo groſs an overſight, as not to be aware, that bodies deprived of their natural qualities, could no more be proper objects of generical relation to each other, in a philoſophic view, than thoſe deprived of certain parts or members, could of ſuch as make the principle of the diſtinction in natural hiſtory. LINNÆUS, in his ſyſtem of diſtribution, has made the generical characters of beaſts, in ſome caſes, depend on the form and number of teeth. Had he unhappily fallen under the criticiſm of my acute adverſaries, they would have ſuppoſed the teeth knocked out of ſome kinds: and where would then have been the difference of theſe, from thoſe which had naturally fewer: and yet this operation, or that of cutting off ears, or tails, in the caſe of Linnæus, are equally allowable with the *powdering* the platina in mine? They are not content, nevertheleſs, with impeaching my definition of earths, by endeavouring to ſhew, that it does not exclude platina, a metallic body; but they ſay further, that it does exclude *chalk*, and all *the calca-*

rious earths. They have not, however, given their reaſons for this: and, I will venture to ſay, either have none, even in their own imagination; or have made ſome other blunder like that of powdering the platina: and, if this latter ſuppoſition be true, and I may take the liberty of gueſſing where the trip has been made; it lies in confounding calcarious ſtones with calcarious earths: and aſcribing to them a failure, in one of the characteriſtics of my ſpecification, from the default of incombuſtibility.

Another charge of this kind is brought in the caſe of gummous and ſulphureous ſubſtances of vegetables; which I have diſtinguiſhed as two different genera; but which, theſe writers ſay, do not differ according to the principles of ſpecification, that I have laid down. They inſinuate in this view, that I have made the diſtinguiſhing character of the ſulphureous to be, that "*they flame in a certain heat,*" and, they ſay, "*how does "this diſtinguiſh them from the gummous, of "which it is alſo ſaid, that in a certain degree "of heat they will flame and glow?*" The whole of this ſeeming defect lies in falſe quotation: and is, therefore,, beſt removed by giving the reſpective paſſages, as they ſtand in my work. In the ſpecification of the gummous ſubſtances, I ſay, page 18. Vol. II. "gums are not of "ſo ſulphureous a nature as to burn through "accenſion, without being previouſly decom- "pounded by heat; for, as they contain no eſſen- "tial oil, they cannot be accended till they be "burnt black: that is, till their conſtituent oil

"be

"be rendered ethereal by the action of heat:" and in the next, page 15, Vol. II: "On their being "subjected to heat with access of air, they do not "(as was observed before) *accend* or *take fire*, as "resins, till they be, in some degree, *decompounded* "*by the action of heat:* and then, after flaming "moderately, and glowing for a *short time*, *ashes* "*are left*, *which*, *besides the earth*, *contain lixivi-* "*ate salts*." In the specification of sulphureous substances, page 21, Vol. II. I make use of these words, "all such substances as may be deemed of a "sulphureous nature have, for their essential cha-"racter, that they will, when heated to a certain "degree with the access of air, burn from the "heat *generated in themselves*, till their whole "*substance be consumed or dissipated*, leaving either "*none*, or but *a very small quantity*, *of ashes or* "*recrement*." It appears, on comparing these quotations, that the specific difference betwixt gums and sulphureous bodies, besides some other variations shewn in other passages, consists in two points; the one, that gums will not accend or take fire, till their nature be changed by the action of exterior heat; and the other, that when they have suffered the utmost effect of heat or fire, there remains not only earth, but lixiviate salt: whereas sulphureous bodies will accend, without previous decomposition through the action of heat; and will support, by the pabulum contained in themselves, a burning state, till their whole substance be dissipated, or only a small quantity of recrement remain.

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Are not theſe ſufficient criterions of the difference of the two genera? but theſe writers, by a ſuppreſſion of one part, and a change of expreſſion in the other, have moſt ſhamefully perverted the clear and obvious ſenſe, to prove that my diſtinctions of genus and ſpecies are imperfect. I hope, nevertheleſs, that what I have delivered of that kind is ſo founded on truth and nature, that it will bear any teſt: at leaſt I may juſtly preſume, from theſe poor efforts, that I may remain ſecure from having the weakneſs of it expoſed by men of the level of genius of my preſent critical adverſaries.

The plan of my work is likewiſe furiouſly attacked; and it is ſaid, that "*however well a work conducted on it might be executed, it would not be a ſyſtem of chemical philoſophy; for ſurely it is not the buſineſs of chemical philoſophy to mould common facts into the form of proceſſes.*" As theſe ingenious writers here, as well as before, have made this work what it was never called by myſelf, nor intended to be, "a body of chemical philoſophy;" they have a right to treat it in that view juſt as they pleaſe: and the reaſon they offer for condemning it when taken in ſuch a light, that "it is not the buſineſs of chemical philoſophy to *mould common facts into the form of proceſſes,*" may be as good as any other; for blunders are moſt ſuitably defended by nonſenſe. They ſubjoin accordingly, that "*the point ought rather to be, to deliver the ſimple truth, diveſted of that inſignificant parade.*" Of the manner of doing which, they have given an excellent example throughout the whole of their own performance: the ſimple truth being every where delivered, according to the genuine ſenſe of my expreſſion, in all their quotations, without *moulding the body into new forms till the ſpirit be evaporated* (if I may borrow an expreſſion uſed by themſelves); or making any parade of deep judgment and ſagacity, in diſcovering what never exiſted but in their own ſophiſtical imaginations. They give, however, one more extraordinary obſervation on my plan, that "*if the ſyſtem were to be completed, it would extend to ſeveral ſcores of volumes.*" By which, if they mean, that if every thing that might be properly inveſtigated by experimental chemiſtry were to be fully examined and recited, ſo many volumes might be filled, I am willing to allow it; but is it an inference, that becauſe there is great extent of ſubject for diſcovery, and a great multiplicity of facts already known, no further improvement ſhould be, therefore, attempted; nor any collection or digeſt of the moſt uſeful part made? It were equally reaſonable to affirm, that becauſe a man cannot know every thing, he ſhould therefore learn nothing.

The directions for the conſtruction of the apparatus, and the conduct of the operations, are in the moſt unfair way cenſured, as being all wrong and improper; and I am moreover condemned for ſaying, that no directions had been hitherto given, for the completely forming and furniſhing an elaboratory for experimental

purpoſes:

purposes: which they intimate to be absurd, on account of the attempts of Glauber, Vigani, Becher, and Dr. Shaw. Whoever knows any thing of the history of that art, must laugh at the three first instances: as they were authors who wrote while the practice of that art was in a most crude and uncultivated state. He might certainly have much better opposed to me some of the late writers, who have had the advantage of the modern great improvements. With respect to Dr. Shaw, I never knew that he had given to the world any thing offered as a complete system of instructions, for the accommodating the utensils and instruments of chemistry solely to experimental purposes; or attempted to teach in what manner an elaboratory for speculative uses only should be furnished: though he has indeed published some accounts of particular parts of such an apparatus, to which I have always ascribed the merit due to them. But after all the defect and failure on this head, which I am charged with by these writers, it seems my capital error lies in the saying, that Windsor loam and Sturbridge clay may be substituted for each other, where only one can be obtained. "*For who that is in the least conversant in chemical experiments, could think, for instance, of taking Windsor loam and Sturbridge clay as equivalent to one another, whether for lutes, furnaces, or vessels.*" I never said they were *equivalent* to each other *for all purposes:* but I advised, what is really practised, that where one could not be obtained, the other might be taken in its place; and the peculiar properties they have in common, and in which they differ from other clays, viz. the being cohesive while in a moist state, and unvitrifiable when exposed to a violent heat, fit them more to be substituted for each other, than any of the substances of the same kind that are to be generally procured. To the end of the last quoted passage they subjoin, "*or of coating retorts for a sand-heat*;". which taken with the preceding part of the paragraph is, "for who, that is in the least conversant with chemical experiments, could think, for instance, of coating retorts for a sand-heat?" Another attempt to shew I was not versed in experiment: but no reference is made to the page where they suppose this is said; and if it be not meant as a delusive falsity, as the omission of the reference usually made by them reasonably suggests, it must be a blundering construction of this passage, where I expresly say the contrary. "The greatest part of the distil-"lations in retorts may be made in a sand-heat, "which indeed is most suitable to them; but "where a very intense degree of heat is required, "the retort being first *coated with a proper lute*, "must be hung in the *open* furnace." Correspondent directions are given in other places: so that there is not the least foundation for the surmise, that I have been mistaken in this point, either from the want of experience, or any other cause. I thought this vindication of myself from the insinuations, that I treated idly and vainly of subjects of which I had no distinct

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knowledge, and where the adequate means of information had been wanting to me, to be neceſſary, with reſpect to thoſe perſons who had not looked into my writings; or were not, from a previous knowledge, judges in this caſe. But to thoſe in the leaſt acquainted with ſubjects of this kind, who have looked into the ELABORATORY LAID OPEN, and the HANDMAID TO THE ARTS, where a great variety of chemical ſubjects are practically treated of, muſt perceive that it is not from the want of an application to experiment in a very extenſive view, that defects of this kind in my works, when ſuch are found, owe their cauſe. Having thus endeavoured to juſtify the plan and execution of the INSTITUTES from the imputations and cenſures of theſe writers in a more general view, I will proceed to do the ſame by particular parts. But not to be tediouſly voluminous, by wading through ſo long a tract of fallacy and abſurdity. I will ſelect a proper number of inſtances of the want of veracity, or intelligence, of theſe writers: which, in order to the more effectually diſplaying them in their true colours, I will diſtinguiſh into two claſſes. MISQUOTATIONS and MISREPRESENTATIONS, or PERVERSIONS OF THE SENSE, and FALSE FACTS or BLUNDERS.

With relation to the firſt claſs, one of the moſt notorious inſtances is found in this paſſage, page 8. of the REMARKS; where they make me ſay directly the contrary of what I have affirmed, in the cleareſt manner of expreſſion, in ſeveral parts of the work: referring moſt confidently to one of them. The paſſage is, "*that calx of biſmuth is not vitreſcible; whereas there is no calx, except, perhaps, that of lead, which vitrifies ſo eaſily.*" So far from denying that calx of biſmuth is not vitreſcible, I ſay, page 264. Vol. II. that "being acted upon by a ſtrong fire, "biſmuth may be ſublimed in flowers: and with "a leſs degree duly continued, converted firſt in-"to a CALX, and afterwards into a VITREOUS "body:" And in the paſſage referred to by them, page 132. Vol. I. INSTITUTES, "the calces of "metals are *vitrifiable* in very various degrees of "heat; thoſe of tin, antimony, and *biſmuth*, are "very refractory: thoſe of copper and iron, of a "more yielding diſpoſition; and that of lead ex-"tremely prone to the vitrefactive change." In both theſe paſſages it is poſitively affirmed, that biſmuth is *vitreſcible*; and though there is ſome variation betwixt what they and myſelf have aſſerted with relation to the degree, comparatively with the calces of ſome other metals; yet whoever will try the experiment, will find it more eaſy to convert copper and iron, than biſmuth, into glaſs. How baſe, how illiberal, for perſons who aſſume the character of gentlemen, to endeavour to work their ends by ſuch fallacies!

Another extraordinary inſtance of a glaring falſity is found in page 6 of the REMARKS, where it is ſaid that in page 310, Vol. I. INSTITUTES, I make the ſpiritus marinus coagulatus to be the ſame with ſea-ſalt: whereas the expreſſion I uſe is this. "If ſpirit of ſalt be combined

"with

" with lixiviate ſalt, a neutral ſalt is produced " GREATLY RESEMBLING ſea-ſalt." Surely the words GREATLY RESEMBLING do not imply them to be the ſame, but different: though this reſemblance is extremely great.

In page 8, REMARKS, I am made to ſay, "*that* " *the pigment, called Pruſſian, is in its whole ſub-* " *ſtance no other than a blue fixt animal ſulphur,*" whereas in the very paſſage referred to, my words are; ſpeaking of the fixt ſulphur of animal ſub-ſtances; " It is this ſulphur which is the ting-" ing matter of the pigment, uſed in painting, " called Pruſſian blue: and which indeed being " combined with the earth of alum, forms with it " the whole of that ſubſtance, when genuinely and " rightly prepared." Page 397, INSTITUTES. Is this aſſerting that the fixt animal ſulphur is the whole ſubſtance of Pruſſian blue, when I expreſsly ſay, ſuch ſulphur is only the tinging matter, and that it contains, beſides, the earth of alum; which is indeed the proper baſis of the pigment? But this is too groſs to need a comment.

In page 8, REMARKS, it is in like manner re-preſented I have ſaid, in page 377, INSTITUTES, " *that the converſion of iron into ſteel depends on* " *the expulſion of mineral ſulphur.*" I do not, indeed, there ſay directly the contrary; but ſomething ſo different, that it makes their aſſer-tion moſt notoriouſly falſe. My words are " the " principle of the converſion of malleable iron into " ſteel by cementation, is ſolely the EXCHANGE of " the mineral ſulphur remaining in the iron for a " purer kind, attracted from the coal in the ce-

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"ment." Certainly there is a great difference betwixt the *expulſion*, a term that does not in any manner occur in the paſſage, and "the *exchange* of one kind of ſulphur for another:" nor can I conſider this, and ſome other ſimilar inſtances of ſuch flagrant miſquotation, as miſtakes, but as deſigned fallacies intended, by the ſacrifice of all truth and honour, to ſerve very bad purpoſes of envy and intereſt.

There is another miſrepreſentation, page 3, REMARKS, of what I have ſaid with reſpect to the fuſion of nitre. "But the place in which it is to be found, is not referred to: for, unluckily, it is not in this work, as I imagine, but in the ELABORATORY LAID OPEN. It is, however, in this paſſage charged upon me, that "*depending on the univerſality of the* "*principle* (that nitre will deflagrate with inflam-"mable ſubſtances) I ASSERT, *that nitre cannot* "*be melted in veſſels made of the deflagrable metals.*" I ſuppoſe by melting is meant fuſion: for, in common uſe, it means, either the liquefaction by heat, or ſolution in fluids: and theſe writers, who ſeem to underſtand the language of chemiſtry as little as the facts, want preciſion in the terms to ſuch a degree, that it is not eaſy to fix the ſenſe of their words, ſo as to form a proper anſwer to what they advance. But preſuming from the context, that fuſion is intended by melting, I do not conceive that ever I aſſerted, in any manner, that "*nitre could not be ſo treated in veſſels made of the deflagrable metals.*" The only place where I can find that I have had occaſion to touch on the fuſion of nitre in metalline veſſels, is in the

the ELABORATORY LAID OPEN, page 181, where I caution againſt the fuſing nitre in the preparation of the ſal prunellæ in veſſels of iron, on account of their deflagrating power; and ſurely this is neither abſurd, nor conveys any falſity, to ſay that deflagrable metals will deflagrate in thoſe circumſtances where that property neceſſarily takes place; and, conſequently, that the veſſels, deſtructible by fuſed nitre, will be ſubject to be deſtroyed by it, in the operation in queſtion. There are many other ſimilar miſquotations, and attempts to miſrepreſent the ſenſe of what I have advanced; but theſe may ſuffice to ſhew the veracity and fairneſs of theſe writers in theſe points. There is, however, one that, for particular reaſons, I beg leave to take notice of: which is what they intimate, page 14 and 15, REMARKS, of my miſrepreſenting and miſtaking Dr. Lewis's experiments on platina; and illuſtrate thus by a pretended inſtance. "*He deſcribes, for example, a proceſs for ſeparating platina from gold by diſſolving the compound in aqua regia, precipitating with fixt alkali, and waſhing the compound. The doctor's experiment, from whence this proceſs is deduced by our author, proves that the platina cannot be ſeparated by this means.*" I am charged with miſtaking and miſrepreſenting Doctor Lewis's experiments; and, in ſupport of this accuſation, an inſtance is produced of my forming a proceſs from one of his experiments, where it does not in the leaſt appear, that I have deviated in any circumſtance from that experiment. But

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I have, it is intimated, applied this proceſs to a purpoſe, which Doctor Lewis's experiment ſhews it cannot anſwer. Certainly, however, this is not either miſtaking or miſrepreſenting Doctor Lewis's experiments; but being miſtaken myſelf with reſpect to the concluſions, I have drawn from the facts evinced by the experiments. I muſt, however, tell theſe peremptory writers, notwithſtanding their aſſertion, *that Dr. Lewis's experiment proves that gold cannot be ſeparated from platina by this means*, the contrary ſeems true from the experiment itſelf; and the principle, on which I found this opinion, is ſo obvious, that any one may comprehend it. Gold being mixt with platina, and the compound being diſſolved in aqua regia, on the addition of alkaline ſalts, both the metals are ſeparated from that menſtruum, and fall to the bottom of the veſſel, in the form of a magiſtery or precipitated powder. So far there appears no means of the ſeparation of the gold from the platina. But the platina being in this ſtate ſoluble in water: if repeated quantities of that fluid be added, and then poured off, the platina being rediſſolved in it, will, by degrees, be thus ſeparated from the gold; which the water cannot in the leaſt diſſolve: and it ſeems, that from thence a method muſt reſult, which might be applied to the ſeparating theſe metallic bodies from each other, in a more groſs way, where great quantities may come in queſtion. It is true, Doctor Lewis did not ſuggeſt this method: mentioning only the facts on which it was founded. But it is neither *miſtake* nor *miſrepre-*

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ſentation of what has been given by him, to make a practical application of a principle deduced from the facts he has related; tho' neglected by himſelf. I ſhall now proceed to the other claſs, and exhibit a few inſtances of the more egregious BLUNDERS and FALSE FACTS reſpecting particulars.

In page 3, REMARKS, he ſays, that I have made "*the attraction of fixt alkalies* (by which I ſuppoſe he means fixt alkaline ſalts, for lime itſelf is a fixt alkali) *greater than to lime or metals*:" and it is very true that I have done ſo. But he ſays, that this "*is true or falſe according to the circumſtances of application.*" I inſiſt, nevertheleſs, on the contrary; and that there is no compound of lime or metals with acids, where a diſpoſſeſſion or depart will not be produced of ſuch lime or metal from the reſpective acid: the proper proof, that the attraction is always greater in my ſenſe. Indeed he has joined another caſe to this, which is, that of phlogiſton and acids, where the attraction does depend on certain circumſtances, as I have ſhewn in table page 27, Vol. IV. INSTITUTES: when, ſpeaking of the attraction of alkalies comparatively to each other, I firſt mention "phlogiſton in the ſtate "of fixt ſulphur in animal and vegetable coal, un- "der that degree of heat which will flux ſalts." Why then is this, and the caſe of lime and metals with acids, joined together, and the obſervation made on them promiſcuouſly, that both are true or falſe according to the circumſtances of application, when I myſelf have explicitly ſhewn the ſame thing with reſpect to the phlogiſton? and it it is not in the leaſt true with reſpect to the other. Is this the conſequence of blunder: or the practice of a mean artifice, effected in this manner? They take two things, of which, one being true, and having been ſaid by me, is admitted to be ſo; and mentioned there as if I had ſaid the contrary; and the other, which, being alſo ſaid by me, is denied to be true againſt the reality of the fact; and they couple them together in ſuch manner, that nothing can be denied or affirmed of the whole propoſition: from whence they conſequently obtain a deception in both points, by making it ſeem that I have aſſerted what was falſe with regard to one; and not delivered what was true with regard to the other.

Another remarkable aſſertion of two glaring falſities is in page 7 where it is ſaid to be "*demonſtrable that Pruſſian blue is iron*; *and that no ſulphur of any kind is contained in animals.*" As this, though the aſſertion of one period, comprizes two very different propoſitions, it is neceſſary to conſider them ſeparately. The firſt, that "*Pruſſian blue is iron,*" may poſſibly have the authority of ſome French writer: and is therefore brought here in contradiction to my poſition, that it is conſtituted of the fixt ſulphur of animal or vegetable ſubſtances, and that earth which is the baſis of alum. It is nevertheleſs falſe, in fact, as may be collected from the manner in which Pruſſian blue may be produced. For though green vitriol, which contains iron, is uſed for the more advantageous preparation of this ſubſtance as a pigment, yet it may be obtained from blood,

blood, or any other animal ſubſtance, and alum, by the aſſiſtance of the lixiviate ſalt and water, without the addition of any matter whatever that contains iron; and therefore cannot be iron, unleſs that metal can be formed by combination of ſubſtances, which are well known not to have the leaſt relation to it. There is, however, a yet more ſimple and certain manner of demonſtrating, that Pruſſian blue is not iron: which is, by practiſing the reductive operation on it: which operation may be thus performed. Commix the Pruſſian blue with powdered coal; and ſubject it in a covered crucible, to the degree of heat that will fuſe iron. When this is done, if the Pruſſian blue was formed of that metal, or any other metallic body which is reducible, it will reſume its proper metallic form, and run into granulæ, that will be eaſily diſtinguiſhed, from the other matter remaining commixt with it, by the aid of a magnet of any ſort. The other propoſition, that "*no ſulphur of any kind is contained in animals*," is delivered by theſe writers, ſolely on their own authority: for certainly none but themſelves could have made ſuch a random, wild, aſſertion. By ſulphur, in the ſenſe I uſe it throughout the whole of my work, and which uſe of it, is juſtified by Sir Iſaac Newton, and indeed almoſt all other modern writers on philoſophic ſubjects; is meant inflammable matter, or that ſubſtance in any compound body, which renders it combuſtible. Now, can any thing be ſo prepoſterous and ab-

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ſurd as to ſay, that there is no inflammable matter in animal ſubſtances? Would not the very candle, by the light of which this ſtrange aſſertion might perhaps be written, evince the contrary; as well as a multiplicity of facts continually before their eyes? But this can only be reſolved, as I obſerved before, into the ignorance of theſe daring writers in the language of chemiſtry: for they certainly confound the PHILOSOPHIC ſenſe of the word ſulphur with the OFFICINAL meaning; where it ſignifies mineral ſulphur or brimſtone. Though had they read the INSTITUTES with care, they could not have fallen into this blunder, as I have always uſed the term MINERAL ſulphur where I ſpeak of brimſtone; and have ſufficiently explained in more than one part, how ſulphur, in the abſtract ſenſe, differs from mineral, or even other fixt ſulphurs.

In page 13, *Remarks*, another extraordinary miſtake appears, in aſſerting that the earths of metals are further decompoundible. The whole paſſage is thus. "*By metallic earths are meant,* "*the common calces of metals, which being all* "*capable of further decompoſition, are, accord*-"*ing to the definition, not earths.*" There is nothing ſo certain, as that the calces of metals, when by that term is meant the earths of metals diveſted of the phlogiſton or ſulphureous part of the reſpective metals, do not admit of further decompoſition. But firſt theſe writers deviate from the ſimple word earths, that I have uſed, and which admits of no ambiguity; and then,

then, having introduced that of calces, fall into one of thofe blunders fo common to them, from a loofe and improper fenfe, in which fome of the German and French writers, and after them perhaps fome Englifh, have ufed that word. For they confound together, under this fame term, the powder obtained by precipitation; which is, in fact, the whole fubftance of the metallic body, only reduced to a pulverine form, and which I have diftinguifhed by the name of magifteries; with the proper calces or earths, that make the bafis of the metals, freed from the fulphureous part. By thus introducing an equivocal and ambiguous word, as fynonymous to that diftinct intelligible one which I have ufed, they have an opportunity of contradicting what I have faid. But I muft infift, that if they take calces in this diffufe fenfe, the earths and calces of metals are not the fame; and that, though it may be affirmed, allowing the magifteries to be calces, that fome of them are decompoundible; yet it will ftill be falfe to fay, that all of them are. For in this fenfe, gold and filver may be reduced to the ftate of a calx; which, neverthelefs, admit of no decompofition. So that in my fenfe of the expreffion "metallic earths," they fay extremely wrong in affirming, that any can be further decompounded: and in the only fenfe in which the term calces of metals can be taken to render it true of them, they are equally wrong in faying, that all can be any way decompounded. Since, taken with this latitude of

meaning,

meaning, it extends to thoſe which cannot. But ſuch blunders are the unavoidable reſult of ſuperficial reading on complex ſubjects; eſpecially by thoſe, whoſe parts are not equal to the ſubtlety and clearneſs of conception required.

Another notorious blunder is made page 13, REMARKS, with reſpect to lime and plaſter of Paris: where I am charged with ſome implied miſtake, in making the ſame ſtones produce both. The whole paſſage is, "*Among the calcarious (earths), or ſuch as* BURN *into lime, are reckoned thoſe which burn not into lime, but into plaſter of Paris.*" But I deny the fact; for I have not reckoned among the calcarious ſtones, ſuch as "burn not into lime;" though I have included ſuch as are by the proper means, though not by *burning*, convertible into plaſter of Paris. The foundation of this falſe poſition of theſe writers, ſeems to lie in two miſtakes. The one, that plaſter of Paris is produced from the gypſeous ſtones, affording it, by *burning*, as lime; whereas it is made by ſubjecting the ſtones to that degree of heat only, which will evaporate the water contained in them. The other, that when the ſame ſtones are urged with a greater and continued heat, ſuch as is ſufficient to convert other calcarious earths into lime, they do not become lime; though they really do. So that in claſſing the ſtones that will afford plaſter of Paris by proper means among the calcarious, I do not "*reckon thoſe which burn not into lime*," but thoſe which really burn into lime; and make therefore a proper part of that claſs. As

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to thoſe which *burn* into plaſter of Paris, they, together with the *decompoundible earths of metals, Pruſſian blue conſtituted of iron, indeflagrable phlogiſton*, and other ſuch ideal ſubſtances, may properly make a claſs by themſelves, in the confuſed imagination of theſe writers. But though I am ſo charitable as to believe, that ſome of the ſeeming miſtakes are the real effects of miſconception; yet I cannot help believing others of them proceed leſs from their own ignorance, than from the deſign of taking advantage of that of others. For the nature of the ſtones affording plaſter of Paris is ſo clearly explained, with regard to the foregoing particulars in the INSTITUTES, that they could not eſcape receiving a ſufficient elucidation if they read the book: which appears from their quotations: unleſs they looked into particular parts, only to lay hold of the firſt paſſages that occurred, to contradict or miſrepreſent it, without the leaſt regard to truth or plauſibility.

In page 8. *Remarks* is aſſerted, in contradiction to what I have advanced, a fact notoriouſly falſe; that ſpirit of wine diſſolves myrrh and amber equally. The whole paſſage is this: they ſay that I affirm "*that ſpirit of wine diſſolves myrrh, and does not diſſolve amber; whereas it really diſſolves one as much as the other, attracting only a part from both.*" That ſpirit of wine will diſſolve myrrh is well known in the common practice of pharmacy, by the preparation of the tinctura myrrhæ: but after many repeated trials, even with the ſtrongeſt alcohol, and aid of other

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media ſaid to produce that effect, I never could diſſolve any ſenſible part of the amber, nor make any other extract than ſuch a proportion of either the amber itſelf, or ſome conſtituent element of it, as was ſufficient to communicate a ſcent to the ſpirit. How bold is it, therefore, in a matter ſo ſimple and eaſily examined, to affirm ſuch a falſity, that ſpirit of wine will diſſolve amber as much as myrrh.

I ſhall only obſerve one more inſtance of blunder, or fallacy, by the aſſertion of a fact believed to be otherwiſe. It is found in page 15, *Remarks*, ſpeaking of the failure of the criterions which I have given for the ſpecification of bodies when applied to ſome particulars. The paſſage is thus, "*The ſpecific character of ethereal oils is, that they riſe with leſs heat than that of boiling water: and yet the oily matter in burnt ſugar and burnt gum is called ethereal, though it will not riſe with double that heat.*" I deny the allegation, that the burnt oil in ſugar and gums will not riſe with the heat of boiling water: though it is true, indeed, that the matter of which the ethereal oil is formed by the action of heat will not riſe with that heat; but it is not in that ſtate in an oleous form. The blunder here, therefore, lies in conſidering that, which will produce by diſtillation in cloſe veſſels the ethereal oil of the ſugar and gum, for the oil itſelf when formed: which is not my miſtake, but that of theſe writers. For I ſay, p. 19, Vol. II. INSTITUTES, that gums, "being "decompounded by heat, on the principle of in-"caleſcence, afford ethereal oil:" and the ſame,

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page 11. ſpeaking of ſugar: and not that they contain ethereal oil in their natural or undecompounded ſtate; for had it been ſo, I ſhould have called ſuch oil *eſſential*. When the oil is produced by the action of heat in theſe ſubſtances, it will riſe afterwards with the heat of boiling water; and is therefore ethereal according to my ſpecification.

As I am accuſed, by theſe writers, of *contemptuous vanity*, with reſpect to other authors; and even of injuſtice, in the characters I have given of them; I think it neceſſary to vindicate myſelf againſt this charge. The method taken by the writers of *The Remarks*, to ſupport this cenſure, has been by collecting all the obſervations that it had been neceſſary for me to make in treating of the different opinions and facts advanced by others, which ſtood in the way of the fundamental truths that were requiſite to be eſtabliſhed in my own ſyſtem: the greateſt part of which obſervations will not be found to concern the general characters of the writers, but only their errors or defects in particular points. But theſe are thrown together in *Italics*, and placed after the names of *Becher*, *Stalh*, and *Boerhave*; as if ſaid of the whole or ſome of them, in the manner quoted. It will appear however, on examination, that I have done great juſtice to all of them: and have aſcribed to each the particular merit they may claim. I have in the preface called *Boerhave*'s work "an ample and valuable collection of the practical proceſſes of chemiſtry:" and have ſaid that *Becher*, and *Stalh*, "were extremely well verſed in experiment;" and that "the latter, eſpecially, greatly extended the knowledge of the relative qualities of ſeveral genera of ſubſtances." A great part, as it ſeems in the repreſentation, of the abuſive manner of treating theſe authors, given in *Italics* as quotation, was not uſed of any author; but are words, which I may have introduced on various particular occaſions, forced together with this malevolent view; and the laſt part was neither ſaid of *Becher*, *Stalh*, nor *Boerhave*; but of *Homberg*, who is now treated in the ſame manner by his own countrymen; and of ſome other authors of memoirs in the academy. The ill manners with which this collection of cenſures is applied to my works, and the abuſive paragraph concluding the pamphlet, deſerve rather my contempt than my reſentment: as they muſt be more advantageous than injurious to me, by illuſtrating in the moſt effectual manner that true ſpirit of malice and deſign that inſpires the writers; and defeating conſequently, in ſome degree, their intention to impoſe on their readers.

I will wave the conſideration of the remaining articles, by which the writers of the REMARKS attempt to ſhew the INSTITUTES OF EXPERIMENTAL CHEMISTRY *to be a contemptible work*, as they have called it; though on their undergoing a like comment, the far greateſt part would prove equally unfair and abſurd, with thoſe we have already examined. I think it proper however to acknowledge, there may be among them one or two miſtakes in point of facts; into which I have, however, been led by con-

confidence in an authority that might well juſtify me: and one inaccuracy, in the ſubſtituting the name of one ſalt for another; which it is obvious, from other parts of my works, was owing to inadvertency, and not ignorance. In all other points touched upon by theſe writers, as faults or defects, I undertake to juſtify myſelf where there is no typographical failure. I do not pretend, nevertheleſs, that it is to the perfection of my work ſolely, that I owe ſo complete a triumph in the vindication of it; the want of knowledge of the ſubject in my cenſurers, and perhaps even that of natural abilities, have been, I muſt confeſs, very favourable to me. I am not ſo arrogant as to imagine, that in a work, of which the deſign is ſo great and new, comprizing ſeveral thouſand articles of the moſt various nature, many of them extremely nice and complex, there is not to be found a ſufficient number of inaccuracies and errors, relating to particular ſubjects, to fill with comments on them, eighteen pages, the quantity of the Remarks: and yet I ſhould not think even this would prove the work to be *contemptible*, conſidered abſtractedly; and much leſs, in compariſon, even to the lateſt publiſhed by others.

I hope, therefore, you will now ſee this *anonymous* pamphlet in its true light, and be convinced, that it was written with the unfair and malevolent deſign of injuring me in your opinion: which the very circumſtances of its being publiſhed at this particuliar criſis, and without a name, are of themſelves ſufficient to ſuggeſt.

geſt. For whoever takes upon himſelf to cenſure books on philoſophic and practical ſubjects, which the author has publickly acknowledged, eſpecially where facts and experiments are in queſtion, ought to put his name to ſuch work of cenſure; otherwiſe there is room to ſuſpect that his motives are ungenerous, and his criticiſm unjuſt; as there can be no reaſon for any man to be aſhamed of ſtanding forth in the cauſe of truth with his face uncovered, when he vindicates her by candid and honourable methods.

I repeat here, that I hope the occaſion will excuſe the liberty I have taken of addreſſing this to you; as it greatly concerns the intereſt I have in your good opinion, to prevent the prejudices intended to be raiſed againſt me.

I am, GENTLEMEN,

With the profoundeſt reſpect,

Your moſt obedient,

and moſt humble ſervant,

R. DOSSIE.